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Author's reply

Gregory Miller and colleagues are correct that accurate calculation of the worldwide prevalence of post-traumatic stress disorder (PTSD) in refugees is not possible. The problem is partly due, as they state, to sample heterogeneity and unreported characteristics. But, in my view, these facts further support the primary focus of my Comment: that accurate measurement of health outcomes in refugees is lacking. Although the systematic review by Fazel and colleagues did not include all possible contextual variables that might cause bias in refugee research, and did not include all possible refugee samples, it was nonetheless novel in pointing out how contextual variables in refugee research create measurement bias.

Miller and colleagues clarify other biases. People from countries of origin that account for most of the world's refugees are often not included in well funded, sound research. Furthermore, although children make up 43% of all refugees, and many refugees reside in less developed countries, they are also not commonly included in research, and so representative data of the world's refugees are not available for systematic reviews such as that of Fazel and colleagues.

The millions of internally displaced people within the borders of their own countries are also very unlikely to be included in good research. In fairness, however, my Comment stated that the article by Fazel and colleagues "adds to what is known about mental disorders in refugees resettled in western countries", and did not imply that these data were representative of all refugees in the world. Nevertheless, the observations by both Fazel and colleagues and Miller and colleagues highlight how important it is that investigators, policymakers, and service organisations recognise the need for more accurate information about refugee trauma and health so that they can make sound decisions about what to do with the scarce resources allotted to the study and assistance of survivors of conflict.

I concede my misunderstanding of the statistics from which I inferred either higher or lower prevalence rates of mental disorder depending on the contextual research variable in Fazel and colleagues' study. The prevalence rates might not vary by the actual percentages I report because, as Miller and colleagues point out, "no predictor is credited with variance shared with other predictors: each correlated predictor no longer measures what it initially measured". I trust that my error does not detract from the importance of Fazel and colleagues' work, which I think shows that contextual variables of the research itself compound the challenges of obtaining accurate prevalence data about the mental health of refugees.

If my Comment incorrectly conveyed that psychiatric prevalence in refugees worldwide is low, then I share Miller and colleagues' concern. In addition to my primary focus being about the inadequacy of methods in refugee research, another message that I wanted to convey is that the resilience of refugees is all too often not honoured or included in research. It is important to know how some who are severely harmed do not develop PTSD or other impairing disorders so that these protective factors might help to guide prevention and treatment strategies. I also make it clear that the mental health effects of war to refugees and others involved in war, whether it is a 10% or a 31% prevalence rate, should be regarded as unacceptable.

If I accurately understand the concern of Miller and colleagues, then I share it: we should use the good scientific methods that we have at our disposal to accurately assess the effects of war on the millions of refugees, asylum seekers, and internally displaced people around the world.

I declare that I have no conflict of interest.

Michael Hollifield
m.hollifield@louisville.edu
Medical Center One, 501 East Broadway, Suite 340, Louisville, KY 40202, USA

Breast cancer

Umberto Veronesi and colleagues (May 14, p 1727)[1] discuss the epidemiology of breast cancer and appropriately state that exposure to ionising radiation at a young age is a widely accepted risk factor for breast cancer. In their table, they define young girls exposed to radiation after age 10 years as a high-risk group. However, two epidemiological studies of radiation-related breast cancer suggest that puberty or adolescence per se is not the prime modifier of radiation-related risk; rather, substantial radiation exposure to the chest at any time before age 20 years—ie, including early childhood—seems to confer a high risk of breast cancer.[2]

The most recent report on breast cancer incidence in Japanese atomic bomb survivors shows statistically indistinguishable, dose-specific excess relative risks of 3·94 (95% CI 1·82–7·60), 1·65 (0·24–4·11), 3·27 (1·89–5·35), and 2·66 (1·59–4·15) per Sv for women exposed at ages 0–4, 5–9, 10–14, and 15–19 years, respectively.[3] In other words, breast cancer risk was high for women who were younger than 20 years of age at the time of the bombings, with little or no evidence for a specific susceptible period within that age window. Furthermore, Land and colleagues[3] showed that risk was significantly lower for women who were older than 20 years at the time of the bombings.

Similarly, a cohort study of childhood cancer survivors included 1258 patients who had received chest radiotherapy, of whom 73 were diagnosed with breast cancer later on.[4] Although the risk of breast cancer was high for these patients, there was no significant difference by age at treatment for the first cancer; the relative risks were 1·6 (0·5–5·1) for exposure at ages 5–9 years

and 1·4 (0·8–2·3) for exposure at ages 10–14 years, compared with the reference group of patients treated when they were 15 years or older.[4]

In summary, these data suggest that a history of radiotherapy involving the chest before 10 years of age merits the same level of concern about subsequent breast cancer risk as a history of exposure at 10–19 years of age.

We declare that we have no conflict of interest.

*Cécile M Ronckers, Charles E Land, Joseph P Neglia, Anna T Meadows
ronckerc@mail.nih.gov

Radiation Epidemiology Branch, National Cancer Institute, Bethesda, MD 20892, USA (CMR, CEL); Department of Pediatrics, University of Minnesota School of Medicine, Minneapolis, MN, USA (JPN); and Division of Oncology, Department of Pediatrics, Children's Hospital of Philadelphia, Philadelphia, PA, USA (ATM)

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Authors' reply

In the first table and in the body of the text of our Seminar, we discuss the association between exposure to ionising radiation and breast cancer risk, concluding that the risk of breast cancer from exposure to ionising radiation seems to be greatest when such exposure takes place around puberty. This period corresponds to about age 10 in girls. Presenting information in tabular form leads to some generalisation and it does not mean, here, that breast cancer risk is not increased when exposure takes place at other ages. This suggestion is confirmed in the Biological Effects of Ionizing Radiation (BEIR) VII report,[1] in which the highest risk of breast cancer was associated with exposure around 10 years of age.

There remains much to be learned about the effect of ionising radiation on breast cancer risk, as shown by the studies cited by Cécile Ronckers and colleagues. An assessment of the effect of the Chernobyl accident on subsequent breast cancer risk in women (of all ages) exposed to radiation is eagerly awaited. However, in terms of the annual global burden of more than 1·1 million incident cases of breast cancer worldwide[2] and more than 371 000 in Europe,[3] the evidence suggests that the contribution of ionising radiation to this burden is small, given the few women exposed at the relevant ages.

We declare that we have no conflict of interest.

Umberto Veronesi, Peter Boyle
umberto.veronesi@ieo.it

European Institute of Oncology, 20141 Milan, Italy

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Improving health workers' performance in low-resource settings

The Review by Alexander Rowe and colleagues (Sept 17, p 1026)[1] on improving and maintaining the performance of health workers in low-resource settings is timely and important. Of the various determinants that influence performance, it is our belief, and that of others working in these settings,[2] that individual motivation and commitment is key to the delivery of good health care. Rowe and colleagues cite several interventions such as incentives, sanctions, and ownership that might improve motivation, but they do not explicitly address the issue of performance-related bonuses.

In 2003, with financial and technical support from the STOP-TB department of WHO, we did a 12-month operational research study in Malawi to determine whether performance-related allowances for central and regional TB programme staff were feasible to implement and associated with improved performance in terms of case finding, treatment outcome, and the sending of sputum specimens for drug resistance monitoring.[3] Allowances were contingent on individual performance, judged by 6 monthly self-assessment forms, and on the TB programme as a whole meeting agreed national targets. Four targets were chosen, the attainment of which reflected improved practice and effort by programme staff. Results were encouraging. Individual self-assessments and performance monitoring of the national programme were feasible to implement, and performance against the agreed targets improved during the study. Unfortunately, the translation of this operational research into routine practice has yet to happen.

The human resource situation in sub-Saharan Africa is dire, with many publications in the past 2 years specifically addressing this issue.[4,5] Training more skilled staff and retaining those already trained in health-care delivery are vital, but will not solve the situation overnight. Getting the most out of the already depleted and overstretched health workforce in resource-poor areas is therefore a priority, and could begin to improve health-care delivery in the short to medium term.

We declare that we have no conflict of interest.

*Anthony D Harries, Felix Salaniponi
adharries@malawi.net

Ministry of Health, PO Box 30377, Lilongwe, Malawi

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